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Book Reviews

The Descent of Manuscripts. By ALBERT C. CLARK. Oxford: Clarendon Press, 1918. 8vo, pp. xiv+464. \$11.20 net.

When the Revised Version of the New Testament appeared (which the late Bishop John Williams, of Connecticut, delighted to call the "Reversed Vision"), prominent among the footnotes stood many which ran, "Many ancient authorities [or 'very many,' or 'some'] read [or 'insert,' or 'omit']" thus-and-so: and many a pious reader was shocked to find banished from the context and relegated to one of these footnotes passages which were sanctified to him by a memory from childhood of the King James Version. He usually knew nothing about "Eastern text," and "Western text," and such-like things. If his curiosity was sufficiently aroused, he was able to discover that the ancient MSS (including versions and quotations) might be roughly classified according as they contained or did not contain certain passages, some long and some short; and that a combat was raging among New Testament critics over the question whether the briefer texts did not in general more closely represent the autographs, the fuller being the product in that respect of interpolations; or vice versa, whether the fuller texts did not better conform to the original writing, the briefer resulting from careless omissions or arbitrary abridgments. The text of the Revisers he might suspect from the footnotes to be somewhat of a compromise in principle between the two extremes; that of the Authorized Version to represent a considerably longer text (though it is not so long as that of the "Western" tradition); and the Greek text of Dr. Hort to exemplify on the whole the extreme of brevity.

In 1914 Albert Curtis Clark, the present Corpus Professor of Latin in the University of Oxford in succession to Robinson Ellis, published a not very large book on "The Primitive Text of the Gospels and Acts" (Clarendon Press, 4s. 6d. net). In this work Professor Clark brought a new sort of criticism to bear on the old controversy about the authenticity of these passages found in one class of ancient authorities and not in others. In addition to considerations which may be passed over here, he pointed out by copious examples that many of these passages are in length the exact measure of a single line in Greek MSS of the most antique type, or are multiples of that unit; and that accordingly, since an interpolator would not be likely to fit his insertion precisely to that pattern of longitude, such passages represent omissions for one or another reason from the archetype rather than interpolations, or congeries of interpolations, into it. He of course did not profess that in every instance the state of the archetype thus determined was surely that of the ultimate autograph,

antedating all possible interpolation, but only that it at least gave these alleged interpolations a reasonably inferred standing of antiquity much earlier, and therefore more trustworthy, than had generally been believed, going back indeed to the middle of the second century. The omission of them from MSS of the shorter text justifies suspicion of either accident or wilful abbreviation as responsible for it. If the MSS that omit them are older than those which contain them, and even though these MSS of the shorter text, by reason of age and perhaps of other excellences, are usually classed as the "best" MSS, the antiquity of the previously suspected passages, now established by the indicated test, but proves the danger of a blind consistency in following in such matters the "better" or "best" MSS and contemning the others. It also proves the fallacy of the old adage *breuior lectio potior* as a universal rule of guidance.

This was of course an argument—and, if valid in full, a powerful one—in favor of the long "Western" text (that of codex Bezae and of the ancient versions and quotations in general) as against the short text (that of the Vatican and Sinaitic codices). The battle of critics was on once more, the gage being taken up forthwith, naturally by champions of the latter tradition. Perhaps the conflict would have raged more vigorously, if a different sort of a war had not distracted the attention of the adversaries at about the time when they were launching their offensive. Professor Clark countered in an article in the *Journal of Theological Studies*, Vol. XVI (January, 1915), pp. 225-40. Meanwhile he had delivered and published (Clarendon Press, 1914, 1s. net) his inaugural address as Corpus Professor on "Recent Developments in Textual Criticism," in which, among other topics, he outlined his method of textual criticism in the matter of omitted passages, reiterated his statement of its necessary and professed limitation of field, and promised the speedy publishing of the further application of it to the text of classical authors, and especially of Cicero, in regard to whose works Professor Clark is pre-eminent among English-speaking critics. This promise is fulfilled in the volume now issued, which, the author tells us, though in print for some time, has been delayed in publication by war conditions.

In the preface and the first three chapters of the book ("Omissions in Manuscripts," "Omission Marks," etc., "The Evidence of Marginalia"), and especially in the third chapter, the author outlines and abundantly illustrates his method. In the ten chapters that follow he applies it; first, to a specimen of patristic literature (Primasius); next, to the Ciceronian palimpsests, orations, and some of the philosophical works; then, to Asconius and the Pseudo-Asconius; and finally, as an example of work on Greek authors, to the leading manuscripts of Plato and the Paris manuscript of Demosthenes.

The underlying idea of Professor Clark's method is of course not altogether new, and does not profess to be so. Omissions and allied phenomena in certain classes or families of manuscripts have often enough been recognized, and have been referred, when they were of sufficient length, to the linear arrangement, and consequently to the length of lines, in an ancestral model. The importance

of Professor Clark's work appears to lie chiefly in its full and minute analytical study of all varieties of such omissions (including repetitions, dislocations, and the like), and in the possibility of applying the process in cases where the omissions are not simply of a word or two, and have been suspected of being mere interpolations. In such cases it may be possible to show, by considerations relating to the age or character of the ancestor to which the omitted passage must ultimately be credited, that there is more reason to suspect that the shorter text has been consciously or unconsciously abbreviated than that the supplied omissions are actually interpolations. Yet here of course the critical sense must not be supplanted or dominated entirely by the measuring rod. That might lead to disaster.

These mathematical computations have an air of imposing conclusiveness. Any appeal to mathematics is likely to appear to the innocent and hesitating classicist to be the welcome struggling out from the morass of wavering and subjective judgments to the solid ground of demonstrable facts. Professor Clark—rather too confidently and jubilantly—professes to rest his case on the mathematics as an unassailable and indefectible witness. He proclaims (p. viii), “To any one who fights against arithmetic I can only say *σκληρόν σοι πρὸς κέντρα λακτίζειν*.” That is superb, but there is no fighting against arithmetic to be feared or suspected. No one is going to doubt that $4 \div 2 = 2$. But what if it does? We may kowtow dutifully before arithmetic, but Professor Clark is not intending to stop with pure arithmetic, though he appears so to claim. He is going on from that to certain deductions that are more tied up with logic and psychology than with mathematical demonstrations. He cannot wind *them* up with a triumphant *q.e.d.* We are entirely justified in accepting the arithmetic but joining issue (if we see fit) on the deductions that go beyond the arithmetic.

The “arithmetical test” may tend to push back the date of the interpolation to an earlier period than that of its first observed occurrence, but it may be an interpolation after all—only of greater antiquity than had perhaps been supposed. The danger of an error in judgment may be greater the nearer the putative date of omission approaches that of the ancient author himself. For when we begin to deal with the period of capital writing in short lines, where the unit of length ranges, let us say, from a minimum of ten to a maximum of twelve letters, altogether too many numbers are readily analyzable into combinations of ten, eleven, and twelve, singly or in multiples, to make the result at all convincing. Thus all numbers of the series 20–24, 30–36, 40–48, and from 50 onward all numbers *ad infinitum*, are thus analyzable. It would be so improbable an affirmation that an ancient book would be written without a single line of less than ten or more than eleven letters, that the similar arithmetical possibilities in such a case are hardly worth enumerating. (They would, however, include the series 20–22, 30–33, 40–44, and so on, and from 90 onward all numbers.) Evidently there are too many possibilities that any given passage might by mere chance exhibit an apparent connection with the

postulated linear arrangement of the early text, when in reality it had nothing at all to do with it. Of course it will be observed that the relative frequency of occurrence of a line of twelve letters in comparison with that of 10-letter or 11-letter lines is of no importance; for granted that a 12-letter line does or may occur even once, the possibility of its occurrence within the limit of the disputed passage cannot be contested, nor any argument validly based that rests upon the postulation of its absence therefrom.

A further difficulty arises when the alleged omission is complete within itself, so that its exclusion does not disturb the sense of the context in which it stood. That is the case with many suspected passages. If it be argued that away back in the period of such short lines or "sense-cola" as have been postulated in this discussion, such omitted passages are very likely by mere chance to measure a certain number of full lines, it is equally likely that an interpolation would also arrange itself by mere chance in similar relation to the unit of length. The probabilities are certainly no greater in the case of the supposed omission than in that of the alleged interpolation, so far as this circumstance is concerned; and it would surely appear more likely in other respects that such a self-contained addition would be an interpolation than that it would be an accidental omission.

Again, it of course cannot be claimed that no omissions of considerable length are possible other than those of a certain number of full lines. Take for example, then, an omission of fifty letters. That may, to be sure, represent an omission of five 10-letter lines; but it may, on the other hand, represent an omission of four and one-half 11-letter lines; and, once granted the possibility of an omission that does not coincide with a certain number of full lines, it cannot logically be postulated that in this specific case (or in any other similar specific case) we must explain the phenomenon one way rather than the other. "But," Professor Clark would probably say, "we have so very many cases where the number of the omitted letters *may* be referable to a certain number of full lines." True: but there is no cumulative probability established in that direction by multiplying cases of the same sort, when in each and every individual case there is no weight of probability on one side rather than on the other. You may add zero to zero indefinitely without the sum arriving at any assignable value. "But," he might reply, "a man is surely more likely to omit, say, five full lines than four lines and a half, and that psychologic fact establishes the probability that the aforesaid fifty letters represent five 10-letter lines rather than four and one-half 11-letter lines." Yes: that establishes a certain degree of apparent probability for the individual case, but the probability becomes no whit greater for any one specific case by the multiplication of the number of cases, so long as every time there is a possibility of the alternative explanation corresponding to the fact. The general probability that a given thing will happen so often out of so many observed times is one thing; the contention, however, that it is therefore probable at any one specific time in the series is quite another thing, and is entirely unjustifiable. The multiplication

of cases produces no increment in the antecedent probability concerning any one case, when no single case can be demonstratively determined. You cannot establish a canon of probability by citing a large number of cases, when each of these cases is itself established only by an appeal to the same canon of probability. That is to argue in a circle.

It should be clearly understood that this is not to deny the determination by a series of uncontested instances outside the range of these cases in question, that a writer is more likely to omit by inadvertence a full line than a line and a half; it is only to deny the contention that by the multiplication of these instances (each one of which is the matter at issue) an overwhelming cumulative probability in favor of each is built up. That is like claiming that, though a plea of "not guilty" by a single defendant to a given indictment creates no necessary presumption of his innocence, the same plea entered by a hundred simultaneous defendants to the same indictment establishes a cumulative presumption (or would Professor Clark even say proof?) in the case of each one that he is innocent. This would be crazy logic trying to disguise itself in the stolen robes of mathematics. To contest such alleged conclusions from arithmetic is not to kick against the pricks; it is merely to puncture an iridescent bubble.

We are back, then, at the point whence we departed, the point that has long been conceded, the psychology of the individual case. All the appeal to the "arithmetical test" might have been spared. That imposing array of arithmetical examples owes all its specious strength to the psychology of the individual case, falsely supposed to be multiplied in effect by the multiplication of assumed instances. On the other hand, each case is subject to the possibility that it may be explicable by the chances that have been pointed out. There is established no presumption, still less such a certainty as Professor Clark appears to assert, that the treated cases are, by reason of their arithmetical relations to certain units of measurement, cases of omission rather than of interpolation. Arithmetic will not settle the problem.

Evidently, for these and other reasons which would require for statement too much space in this journal, the text-critic need not fear that the abacus is about to supplant his trained judgment. He may therefore suppress any bristling of jealous apprehension, and study the new book and its antecursorial satellites with equanimity, and with gratitude for what he may find there of use and stimulus. Even if he does not agree with the author in his main thesis, he will yet be repaid by the large stores of knowledge that are here put at his disposal, stores not at all conditioned by their immediate relation to the author's special purpose. Neither palaeographer nor text-critic can afford to neglect the book, or merely to skim it. It is one of enchainning interest. To the younger student on the search for problems it opens a broad field, over which the author does not desire to assert proprietary rights of original pre-emption. "The material," he says, "is inexhaustible," and he gives would-be investigators, not merely a complete manual with extensive examples of his

method, but also (pp. xiii f.) practical advice about how to attack similar work.

One bitter word in conclusion about the price of the book. The volume is not very large, nor is it very elaborate in type. There are no plates or other facsimiles. The distributor (presumably the office-boy of the American Branch of the Oxford University Press) has tried the silly ostrich-like trick of cutting out from the printed wrapper the statement of the English price (it was 28s., as any schoolboy could easily discover), and has penciled in the price demanded in this country, \$11.20!—a rate of forty cents to the shilling. The English price is appalling, the American outrageous. Of course it will grievously limit the sale of the book, and it will not engender very kindly feelings toward the American agents.

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